

I have been working and have published on several of the taxa mentioned by the authors of Case 3061; I fully support their conclusions and application and recommend that the Commission accepts their proposals. However, I note two minor mistakes. The first is only a detail, and the second is remedied by a lectotype designation which contributes to the nomenclatural stability within this group.

Ng et al. write (paras. 2 and 4 of their application) that *Bagrus planiceps* Valenciennes, 1840 was described from two specimens collected by Kuhl and van Hasselt. I assume this was based on Valenciennes's remark 'nous en avons vu de quatre et de huit pouces de longueur', but this could encompass more than two specimens; Valenciennes clearly stated that there was one specimen in Paris and others in Leiden, and this is corroborated by the present holdings of those museums (see para. 4 of the application). This detail does not change anything about the need for a lectotype designation for *B. planiceps*, as made by the authors in para. 10 of the application.

Ng et al. also write in paras. 2 and 4 that *Bagrus anisurus* Valenciennes, 1840 was based on a single specimen, i.e. a holotype. I disagree. The description starts [in translation] 'Messrs Kuhl and van Hasselt have had a third bagre painted in Java, of which they have sent samples [plural] to the museum in Leiden etc.'. Valenciennes did write in the account of the species 'The individual which we have described is 14 inches long', but the specimens in Leiden were included in the species and are therefore syntypes. Furthermore, the description ends 'In the liquor [alcohol], it appears pale brown on the back, and whitish grey under the belly; but when fresh as in the figure, the whole upper part is olivaceous', and there is no reason to suppose that both parts of this sentence refer to a single specimen painted when fresh and then preserved and now in Paris.

In line with the argument by Ng et al. that the names of the nominal species now in *Hemibagrus* should be defined, I here designate the specimen NNM 2956 in Paris as the lectotype of *Bagrus anisurus* Valenciennes, 1840; this is the specimen assumed by Ng et al. to be the holotype.

As stated at the outset, I support the proposals in the application by Ng et al.

#### **Comments on the proposed conservation of the specific name of *Varanus teriae* Sprackland, 1991 (Reptilia, Squamata)**

(Case 3043; see BZN 54: 100–103; 250–251; 55: 37–39, 111–114)

(1) H.G. Cogger

*clo The Australian Museum, 6 College Street, Sydney South,  
New South Wales 2000, Australia*

Rather belatedly I wish to comment on this application, submitted by Profs R.G. Sprackland and H.M. Smith and Dr P.D. Strimple in BZN 54: 100–103 (June 1997).

Although the 'Code of Ethics' (Appendix A in both the 3rd and 4th editions of the Code) and many of the Code's important Recommendations were blatantly flouted in the Wells & Wellington (1985a) work at the core of this case, leading many workers to reject all or part of the publication, the Code of Ethics and Recommendations are not mandatory. The Commission noted (BZN 48: 337–338, December 1991) that 'the provisions of the Code apply to all names directly and indirectly involved in this

[Wells & Wellington, 1985] case, and that it will be guided in future submissions by the criteria of usage, nomenclatural stability and the views of the zoological community which it serves'.

Because both specific names of *Odatria keithhornei* Wells & Wellington, 1985 and *Varanus teriae* Sprackland, 1991 are young and both are in use, the choice of either name will not impact on stability or universality of nomenclature, and so there is no basis for invoking the plenary powers. Therefore the mandatory provisions of the Code should apply, with the senior synonym (*Varanus keithhornei*) being confirmed as the valid name of the taxon.

(2) R.G. Sprackland

*Young Forest Company, 951 Old County Road Suite 134, Belmont, California 94002, U.S.A.*

H.M. Smith

*Department of Environmental, Population and Organismic Biology, University of Colorado, Boulder, Colorado 80309-0334, U.S.A.*

P.D. Strimple

*Reptile Research and Breeding Facility, 5310 Sultana Drive, Cincinnati, Ohio 45238, U.S.A.*

In answer to previous comments on this case, we wish to reiterate that the second Wells & Wellington publication (1985a) was unobtainable via several libraries at the time (1982-1989) that one of us (R.G.S.) undertook revisionary work on the *Varanus prasimus* species group of monitor lizards. In our view this is more relevant than the fact that a few people had copies. We suspect that most copies were distributed after Sprackland's own (1991) publication. Why, otherwise, could no major library provide either 1985 Wells & Wellington paper when he did his literature searches; did no museum have anything other than the 1983 Wells & Wellington publication; and did the Queensland Museum, who published Sprackland's paper (1991) after a number of alterations requested by reviewers, not inform him that a name for the tree monitor from northeastern Australia, based on specimen QMJ31566 in the Queensland Museum, had already been published?

The choice of specific name for the tree monitor is between *Varanus teriae*, which is now eight years old, and *V. keithhornei*, now 14 years old. The synonymy between the two names was not realized until 1994. In the time that *teriae* has been published it has had considerable usage, which has continued since recognition of the synonymy (see para. 3 of the application; to the list of references may be added Reháč & Velenský, 1997).

Cogger (above) seems to think that because both names are relatively recent stability is not at stake. He fails to realize the importance of his own works, which regularly used the junior of the two names, *V. teriae*. Stability is a product not only of frequency of usage but also of the influence thereof. Cogger's works are the most important guides for biologists in general to the herpetology of Australia, and thereby are of much more significance than little-noted, incidental usages. And there is where the weight of stability rests.

In our view, and contrary to that of Drs T. Ziegler and W. Böhme (BZN 55: 112), the ability to use stable nomenclature for the inclusion of species and subspecies in CITES and other conservation legislative documentation is an important issue. Taxonomists are the servants of all those who use scientific names and work to serve those needs, not to establish an authority to which everyone must subscribe whether in accord with stability or not. We believe that our aim must be to provide an environment of nomenclatural stability in which biologists may work with confidence.

#### Additional reference

Rehák, I. & Velenský, P. 1997. Biology of the varanids *Varanus prasinus*, *V. rudicollis* and *V. salvadorii* in captivity. *Gazella*, **24**: 108–138. [In Czech; English summary].

**Comment on the proposed suppression of all prior usages of generic and specific names of birds (Aves) by John Gould and others conventionally accepted as published in the *Proceedings of the Zoological Society of London* (Case 3044; see BZN 54: 172–182; 55: 176–185)**

(1) Murray D. Bruce and Ian A.W. McAllan

*P.O. Box 180, Turramurra, New South Wales 2074, Australia*

We are the authors of the original paper under consideration as Case 3044. Various points covered by Schodde & Bock (1997), the comments of Olson (1998) and the response of Schodde & Bock (1998) [as cited above] require further comment. It should also be noted that our paper, although dated 1990, was published in 1991, as pointed out by McAllan (1992).

1. Inconsistencies in the use of reports published in *The Athenaeum*, *The Literary Gazette* and *The Analyst* prompted our review of these serials. The first two were of considerable importance for many years as general sources of information covering the sciences and other fields. The third was a short-lived journal from the 1830s and one of several from this period affecting zoological nomenclature. As an example of inconsistency, we pointed out that although *The Athenaeum* is accepted for *Balaeniceps rex* (a very brief but adequate description) in a standard work (Kahl, 1979), there were other names variously mentioned or overlooked, with equal claims to priority. Also, we deplored the proposal for suppression of a name from *The Literary Gazette* without the actual reference being examined (LeCroy, 1988; LeCroy & Bock, 1989), an action invalid for other reasons, as we discussed (Bruce & McAllan, 1991).

2. The latter example prompted us to provide verbatim extracts of the relevant references in our paper to facilitate an evaluation of our findings and to avoid the argument of the rarity or inaccessibility of the sources (a pointless criticism in view of the rarity and inaccessibility of many sources long accepted in avian nomenclature). We found hundreds of nomina nuda in our investigations but only discussed those names identifiable by descriptive details. For example, we did not discuss *D[inornis]*, *dromaeoides* because it is a nomen nudum in *The Literary Gazette*. The only nomen nudum we did discuss was *Sitta ferrugineoventris* in *The Athenaeum*